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world is "mind-stuff" or is "will." It can manifestly have no place in a doctrine in which human beings as well as human interests and values are infected with self-contradictions and can only contribute to reality in so far as they "cease." Is there any such conservation, in an unequivocal sense of the word, in the world attained by the expansion of the "internal meaning" of the song? I can find none whatever, though I am aware that others think they do. The cheerful pragmatic doctrine does make room for it—its protagonist was a realist—and in this respect it may be classed with the idealism of Berkeley, and contrasted with the other philosophies examined.

The moral of all which seems to me to be that, in making up our minds what sort of a world this or that writer believes our world to be, we must not ask merely whether he is a realist or an idealist. Idealists differ vastly among themselves. So do realists—even new realists. I ought to have made this clear in my book, if I have not done so. And if any idealist does in his philosophy provide unequivocally for the conservation of human interests and values, I am inclined to think that he is moved to do it by much the same considerations that influence the realist who makes the same provision, however different the paths which they seem to follow. In so far they ought not to be out of sympathy with each other.

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REVIEWS AND ABSTRACTS OF LITERATURE

Das Problem der Vererbung "erworbener Eigenschaften." RICHARD SEMON. Leipzig: Wilhelm Engelmann. 1912. Pp. viii + 203.

It is about a generation since Weismann precipitated the famous controversy over the "inheritance of acquired characters." Prior to that time such a mode of inheritance was generally accepted without question; indeed, the distinction drawn by Weismann between "germinal" and "acquired" characters had not even occurred to most biologists. The problem was promptly recognized as one of far-reaching importance. Herbert Spencer rightly saw that the triumph of this new point of view meant the collapse of some of his most cherished theories. For some years the controversy was waged fiercely. But at that time little appeal could be made to experiment, though Brown-Séquard's guinea-pigs long did valiant service on the one side and Weismann's dock-tailed mice on the other.

Weismann's skill as a dialectician, together with the real scarcity of decisive evidence on the other side, led to a strong trend of opinion in his favor. This trend gained further support from the brilliant studies of the cell which held the attention of biologists during the years when the controversy on "acquired characters" was keenest. One discovery after another seemed to speak for the immutability of the germinal substance and the permanent individuality of its constituent parts.

Then came the unearthing of Mendel's celebrated experiments, and the formulation of the "mutation theory" of De Vries. Experimental Mendelism attracted hosts of enthusiastic followers, who busied themselves with elaborating the Mendelian formula to the *n*th power. And an immensely important mass of facts they have undoubtedly brought to light. To this dominant school of investigators, any suggestion that the old question of "acquired characters" was still deserving of an experimental test seemed reactionary or worse. "Lamarckism" to many of this school of thought was beyond the pale of scientific discussion altogether, being hardly more respectable than the alleged phenomena exhibited at a spiritualistic séance.

But, fortunately, a small group of investigators have not been deterred by considerations of "respectability," and now, after some fifteen or twenty years of experimentation, they have amassed a body of evidence which can no longer be ignored or treated lightly. *Evidence for what?* Certainly for the "inheritance of acquired characters" in the sense in which that expression was employed at the commencement of the controversy. But to many the time has come for discarding that rather threadbare expression altogether, partly because it is, in its nature, ambiguous, partly because Weismann and his followers have progressively restricted its meaning so as to exclude each new fact which they have been compelled to accept.

A few years ago, Semon brought together in a comprehensive way the most important data supporting this mode of inheritance in a paper of some 80 pages.[1] Very recently, he has amplified this earlier work and extended its scope. As a mere summary of a certain important group of biological facts, this little book is invaluable. It is most complete in its record of those experiments which have had for their object a direct attack upon the main problem which we are here considering. But nature's own experiments have in some cases received full consideration. Evidence has been drawn, for example, from the thickening of the skin on the soles of the feet, a condition already evident in the fetus, and from the diurnal rhythm of leaf-closing in certain plants, which is independent of individual experience.

Evidence, we may ask again, of what? Semon himself formulates his problem as follows: "May we suppose that, under favorable circumstances, through reactions evoked in the parent body, the hereditary potencies of the germ-cells (genotypical constitution), and thereby the reaction-mode of the offspring, may become modified in the same direction as the modifications appearing in the parents?" (pp. 9, 10). This may sound like a clumsy and not very intelligible way of asking whether or not acquired characters are inherited. But real differences are expressed. It is not an inhertance of the "character" itself which is contended for, but an altered *disposition,* which leads the "character" to develop spontaneously at a given time or place. The "character" is merely an outward mark or symbol, through which the response of the organism to some stimulus manifests itself. The absence of such an outward indication in the parent does not necessarily imply the lack of any sort of reaction on the part of its living tissues. On the other hand, the presence of such an outward manifestation, though indicating some sort of reaction to a stimulus, on the part of the organism affected, does not necessarily imply that this reaction occurred at a time when the germ-cells could be influenced. To state the matter somewhat paradoxically, we may have the inheritance of an "acquired character" which was not manifest in the parent at all; while, on the other hand, a manifest character may have been impressed upon the parent at a period when any transmission was out of question (pp. 7, 8). Here, and elsewhere throughout the book, Semon makes use of Tower's apparent demonstration of the existence of a particularly impressionable period in the life of the germ-cells. Influences which are brought to bear during this period may lead to perceptible results in the offspring, while influences acting at any other time will probably have no appreciable effect. This is one explanation which Semon offers for the admittedly large proportion of cases in which modifications of the parent body leave no trace in the offspring.

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[1] "Der Stand der Frage nach der Vererbung erworbener Eigenschaften," *Fortschritte der naturwissenschaftlichen Forschung,* II. Band, 1910.

It is plain to any one who follows the present work that the problem dealt with is one of much greater complexity than had been supposed at the time when it was first formulated by Weismann. Indeed, it is well known that Weismann's own conception of the "continuity of the germ-plasm" soon lost all claim to simplicity, but annexed one subsidiary hypothesis after another, as fast as data were produced which were incompatible with the earlier versions. Weismann was forced to admit not only the reality of a general influence upon the germ-cells of conditions affecting the "soma" or body, but to recognize the possibility that the former might be influenced locally and partially, resulting in definitely restricted effects upon the offspring. Such effects would not necessarily resemble, however, the original effects upon the parent, and even if they should, *there would be no inheritance of acquired characters.* For such instances of the parallel modification of parent and offspring, through the influence of external conditions, Weismann made use of a hypothesis to which the name of "parallel induction" was later given by Detto. This is the simple assumption[2] that the germ-cells are directly reached by the same physical influences, *e. g.,* temperature or nutrition, which bring about the modification of the parent body. Plainly, in such an event, it would be improper to speak of *inheritance* at all. This hypothesis has been used very freely by Weismann and others, and notably by Tower, who has brought forward some seemingly strong evidence in its support.

Among the most convincing parts of Semon's discussion are those directed against this interpretation of the phenomena at hand. He rightly contends that in some of the best authenticated cases of the repetition of parental modifications in the offspring, the possibility of a simultaneous influence upon the germ-cells is practically excluded. Consider, for example, the effects of optic stimuli upon a salamander, or of temperature stimuli upon a mammal, or again of habituation to contact with mankind on the part of a wild species.

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[2] The simplicity is more apparent than real, as the present reviewer, among others, has pointed out. (See *American Naturalist,* February, 1911, page 96.)

And even in those cases where the same physical agent could conceivably influence both the soma and the germ-cells, there are serious physiological difficulties in the way of thus accounting for identical specific effects upon parent and offspring. Here, as elsewhere, it is likely that the germ-cells are affected, not immediately, but secondarily, through a reaction evoked in the parental organism.

Semon has devoted considerable space to answering some of the older arguments against the "inheritance of acquired characters" which were based upon negative evidence, *i. e.*, the failure to detect such an inheritance in what would seem to be crucial cases. As regards mutilation experiments, for example, his answers are various. Perhaps, he says, some of these experiments would have yielded positive results if performed during the impressionable period of the germ-cells. And, after all, we must remember that a mutilation is not a reaction to a stimulus on the part of the organism, but a mere passive effect. The typical response to an injury, when such occurs, is regeneration. And here he points to certain experiments of Kammerer's upon the ascidian *Ciona,* in which it was found that the enlargement of the siphons, after amputation, repeated itself in the succeeding generation (pp. 58, 59).

Two seemingly formidable objections, which have been raised of late to the inheritance of parental modifications, are derived from recent work in experimental breeding. The first of these is the observed fact that "pure recessives," in cases of Mendelian inheritance, appear to be as pure, when extracted after many generations of hybridization, as they are at the outset. Why have not the "dominant" soma characters of the parent affected the eggs or sperm cells? Semon's attempt to meet this difficulty is, it must be confessed, not entirely satisfying, since it rests upon an assumption which does not seem wholly warranted. At other points in his discussion, however, he contends that "morphogenic excitations (those conditioned by the mere presence of parts) are, as a rule, perhaps everywhere, too weak to bring about an induction of the germ-cells" (p. 171). The most effective modifying influences for the latter are *active reactions* of the parent body to this or that external stimulus. "Functional excitations" (*i. e.,* such as arise from the use and disuse of parts) stand, in Semon's opinion, intermediate between the other two classes.

The second of these difficulties is one which has been made much of by Johannsen, namely, that the artificial selection of ordinary quantitative variations, particularly within a "pure line" (strain of uniform heredity composition) appears to be entirely inefficacious. Since the differences which appear within a pure line are, it is assumed, due wholly to the effect of chance differences in the environment of the various individuals, this fact is naturally offered as an additional argument against the "inheritance of acquired characters." The reply is that long-continued breeding is perhaps what is necessary in order to produce any visible results in such cases, and furthermore, that the selection of "quantitative" or "fluctuating" variations has, in reality, shown itself to be efficacious in the hands of such able breeders as Cuénot and Castle. Finally, negative results of this sort have no conclusive weight. There are confessedly many other cases of parental modification which appear to be without any visible effect upon the offspring, however long repeated.

In a controversy like the present one, where there are several interwoven problems in dispute, and where much of the dispute relates to the definition of terms, it means little to assert that either one or the other side of the case has been "proved." We may, however, heartily concur in Semon's belief that henceforth "any theory of heredity or descent has to reckon with the empirically established factor of somatic inheritance" (p. 173), without, perhaps, wholly agreeing with him in his interpretation of this "somatic inheritance." Indeed, we may go further and add that the dogmatic and unqualified denial of the "inheritance of acquired characters," which is still frequently heard, implies not only an ignorance of many well-attested facts, but an unfamiliarity with the present status of the whole question discussed in the work before us.

It is hardly fair to close the review of even such an admirable book as this one of Semon's without pointing out what seem to be one or two weak points. To the reviewer it appears that the worth of the experimental evidence has not always been carefully weighed. Most of us will probably agree in our admiration for the amazing productivity and versatility of the Austrian zoologist Kammerer. Yet the very extent of the territory covered by this investigator within the space of comparatively few years would deter many of us from basing any weighty conclusions upon some of his unconfirmed experiments.

Again, the possibility does not seem to be fully considered by Semon that detailed correspondences between parent and offspring may not always imply the existence of a causal nexus as complicated as the phenomena which it links together. Pathology teaches us that the action of a single bacterial poison may produce quite an elaborate symptom-complex, requiring weeks for its full development. Why may not the transmission of a comparatively simple chemical substance from parent to offspring produce in the latter a "symptom-complex" which parallels in detail a similarly caused group of changes in the former?

Thus it might turn out that some of the known instances of "somatic inheritance" could be explained in relatively simple terms. Whether or not such an explanation is of general applicability may well be doubted, but it ought, at least, to be fully considered before phenomena of a less conceivable sort are invoked. In any case, however, it would seem a mere quibble to deny the use of the word "inheritance" to such a form of transmission.

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